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# Punitive welfare reform and claimant mental health: The impact of benefit sanctions on anxiety and depression

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**Abstract**

Internationally, policymakers assume that sanctioning claimants of unemployment benefits will engender both improved employment outcomes and wider positive effects. A growing evidence-base challenges these expectations, though additional insight is needed from large-scale longitudinal research. This article contributes by conducting a quantitative investigation into the mental health impacts of benefit sanctions. To do so, it focuses on a recent period in UK sanctions policy in which rates of sanctions varied markedly and their length was substantially increased. Using quarterly panel data for local authorities in England (Q3 2010–Q4 2014) and fixed effects models that control for important confounders, the analysis provides robust evidence that Jobseeker's Allowance (JSA) sanctions lead to increases in self-reported anxiety and depression. Evidence of this adverse impact is particularly clear following the increase in the length of sanctions in October 2012. The results have important implications for contemporary social security policy, which is underpinned by a similarly punitive sanctions regime. Whilst additional individual-level research is needed to fully consider the causal relationships in operation, the findings support a precautionary approach that should seek to minimise the harm associated with sanctions.

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This implies taking steps to reduce both the severity and frequency of applied sanctions.

**KEYWORDS**

benefit sanctions, conditionality, mental health, social security, welfare reform

## 1 | INTRODUCTION

Benefit sanctions, in the UK and internationally, are used to enforce the conditions of unemployment benefit receipt (Immervoll & Knotz, 2018). When claimants are deemed to have contravened specific requirements, they face a reduction or complete withdrawal of their benefit income for a period of time. Policymakers typically assume that sanctions will lead to increased transitions into employment, an effect that is itself expected to bring about wider benefits such as improved health and reduced child poverty (DWP, 2011, 2018b). An emerging evidence-base contests these assumptions, however, in the areas of both labour market and wider impacts. This article focuses specifically on the issue of mental health and carries out a quantitative investigation into the impacts of sanctions on anxiety and depression.

Using data for Jobseeker's Allowance (JSA) sanctions applied in England, the analysis considers developments in sanctions policy during the Coalition government (2010–15). This period is of substantive interest, since it was marked by an historically high application of sanctions and large fluctuations in their frequency, as well as an increase in their length from October 2012 onwards (Webster, 2016). This latter reform is of continued relevance within the UK's social security system, since it underpins the enforcement of benefit conditions for Universal Credit (UC). Indeed, sustained criticism of the 2012 reform has motivated several official investigations into sanctions policy. The most recent of these concludes that, given its failure to do so in the intervening period, the Department for Work and Pensions (DWP) needs to “urgently evaluate the effectiveness of reforms ... introduced since 2012” (HoC WPC, 2018, p. 19).

In light of this, this article conducts an investigation that contributes to existing knowledge of the impacts of the 2012 sanctions reform. To do so, the analysis focuses on the period prior to the national rollout of UC in 2015, which is necessary to avoid measurement and data availability issues brought about by its introduction. A fixed effects regression analysis is carried out that uses panel data on JSA sanctions and self-reported anxiety/depression at the local authority-level. The remainder of this article is split into four parts: Section 2 considers existing evidence on the impacts of benefit sanctions; Section 3 outlines the data and methods used in the analysis; Section 4 describes the results; and Section 5 provides a discussion and conclusion.

## 2 | THE IMPACTS OF BENEFIT SANCTIONS

### 2.1 | Labour market and wider impacts

Benefit sanctions are financial penalties that are used to enforce the conditional requirements of benefit receipt, which together regulate both initial and ongoing access to state-provided support (Watts & Fitzpatrick, 2018). In the case of unemployment benefits, conditions have long required that claimants be available for work and that they did not leave their previous job voluntarily or due to misconduct (Adler, 2018). In recent decades, however, unemployment has been re-conceptualised in terms of “job-seeking,” with claimants now required to “actively” seek work through monitored job-search and additional work-related activity (Boland & Griffin, 2015). Knotz (2018) identifies

an increased reliance on conditionality and sanctions across the OECD from the 1990s onwards. Claimants have come under greater pressure to conform to job-search requirements and accept potential employment, enforced through sanctions for repeat refusals of work. Amongst this overall trend, Knotz (2019) highlights Austria, Finland, Ireland, Italy, Sweden, and the UK as countries that have experienced notable increases in the strictness of sanctions, though rarer instances of decreases have nevertheless been observed in Australia, Germany and New Zealand.

An academic literature has emerged that investigates the labour market and wider impacts of sanctions policy. With regard to labour market effects, emerging conclusions are identified in existing reviews of an extensive international quantitative literature (Griggs & Evans, 2010; McVicar, 2014). There is consistent evidence that, in the short-term, sanctions are associated with increased exits to employment. Severe financial penalties, however, are not needed for this impact to occur; in one study, 5% reductions in benefit income are found to be just as effective as reductions of 10% or 20% (van den Berg, van der Klaauw, & van Ours, 2004). This is supported by existing research in the UK, which finds no evidence that the lengthier sanctions introduced in October 2012 had a positive employment impact (Taulbut, Mackay, & McCartney, 2018). Sanctions are also found to increase exits out of the labour force, an effect that is often larger than the observed employment effect (Arni, Lalive, & Van Ours, 2013; Busk, 2016). In the longer-term, the evidence indicates that for those who find work, sanctions are associated with adverse outcomes on job quality in terms of occupational mismatches, lower earnings and reduced job duration (Arni et al., 2013; Hohenleitner & Hillmann, 2019; van den Berg & Vikström, 2014).

Policymakers assume that additional benefits of sanctions policy, for both claimants and wider society, will follow from increased transitions into employment. The above evidence complicates this view, however, given that employment outcomes for sanctioned claimants are far from universally positive. There is a risk, therefore, that sanctions policy will in fact be associated with adverse wider impacts. Indeed, a developing quantitative and qualitative literature—primarily UK and US based—highlights a range of negative outcomes that are associated with sanctions (Dwyer, 2018; Griggs & Evans, 2010). These include, though are not limited to, impacts in terms of: debt and financial hardship; rent arrears, eviction and homelessness; survival crime; domestic strain and impacts on friends, family and children.

The empirical focus of this article is on mental health impacts, using data that relate to JSA benefit sanctions, previously the UK's main unemployment benefit. In the UK policy literature, existing departmental evaluations of JSA sanctions highlight a range of emotional and psychological impacts, which typically include: stress and panic; anger, frustration and humiliation; and low mood, anxiety and depression (Peters & Joyce, 2006; Saunders, Stone, & Candy, 2001; Vincent, 1998). This evidence is supported by qualitative research, which similarly highlights mental health impacts for the unemployed as well as claimant groups such as lone parents and disabled people (Dwyer, Scullion, Jones, McNeill, & Stewart, 2020; Jamieson, 2020; Wright, Fletcher, & Stewart, 2020). In one such study, the authors detail "severe and acute negative emotional effects" (Wright & Stewart, 2016, p. 4) for JSA claimants following from both the threat and imposition of sanctions. Existing quantitative research in this area, furthermore, links benefit sanctions to impacts on mental ill-health in the US and antidepressant prescribing in the UK (Davis, 2019; Williams, 2019). This article contributes to this limited quantitative literature through an investigation into the relationship between JSA sanctions and anxiety/depression.

## 2.2 | UK policy context

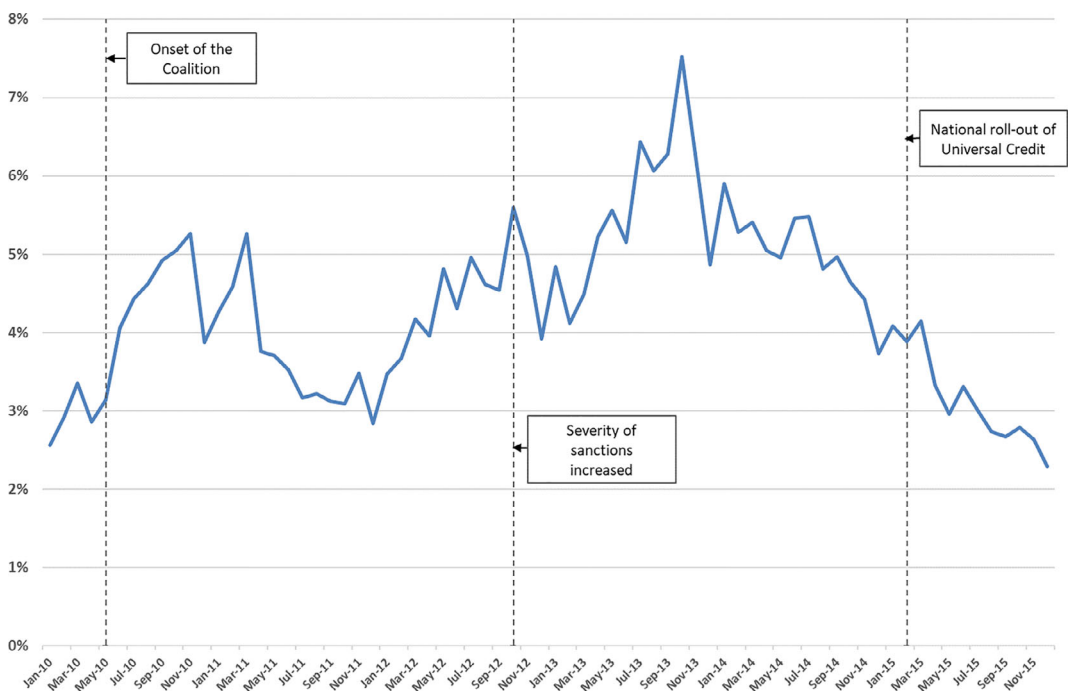
The above discussion indicates that the social security system has an important role to play in influencing—for better or worse—the existing adverse impact of unemployment on mental health (Paul & Moser, 2009). In the UK, current social security policy is dominated by the ongoing rollout of UC. UC was introduced in pilot form in 2013, followed by a national rollout in 2015 that is not expected to be completed until at least 2024 (Kennedy & Keen, 2018). It is designed as a single working-age benefit that replaces six pre-existing means-tested forms of state support, within

which work-related conditions and sanctions apply to individuals in a range of different circumstances: the unemployed; lone parents; disabled people; and people in low-paid or part-time employment (Millar & Bennett, 2017).

The empirical investigation in this article focuses on developments in the period prior to the national rollout of UC, which oversaw a punitive shift in UK sanctions policy (Wright et al., 2020). This period is of substantive interest, since benefit sanction reforms made by the Coalition continue to underpin the enforcement of conditions within the contemporary social security system. With regard to the average length of JSA sanctions, which was the main unemployment benefit that was replaced by UC, a new regime was implemented in October 2012 that increased the minimum and maximum length of a sanction to 4 and 156 weeks respectively. Representing full JSA loss for these periods, these sanctions compare to the previous bounds of 1 and 26 weeks. Ultimately, the exact length of a sanction that an individual receives depends on the type and number of rules contravened, as detailed in online Appendix 1 (Table A1). The new rules codified rapid increases in sanction severity for second and third infringements, a steep escalating structure that is unusual in comparison to other OECD countries (Immervoll & Knotz, 2018).

As Figure 1 displays, the monthly sanction rate also varied considerably between 2010 and 2015. The rate of sanctions increased from the onset of the Coalition in May 2010 to April 2011, at which point a “system of targets through benchmarks” (Couling, 2013, p. 3) was removed, leading to a fall in sanctions to December 2011 (Webster, 2016). From April 2011, new performance indicators for Jobcentre Plus offices and staff were introduced whereby sanction referral rates formed part of advisors' overall performance assessment. A subsequent investigation by the National Audit Office (NAO, 2016) highlights evidence from Jobcentre staff who experienced increased management pressure to sanction claimants in the period leading up to the height of applied sanctions in October 2013, followed by a relaxation in such pressure and an observed decline in the sanction rate.

Another important determinant of sanction rates during the period is the Work Programme, the Coalition's flagship welfare-to-work scheme (de Vries, Reeves, & Geiger, 2017). The Work Programme led to an increase in the number of sanction referrals because providers were obliged by the DWP to refer claimants for very marginal



**FIGURE 1** Monthly rate of JSA sanctions (per cent of JSA claimants), line graph

Note: Author's calculations using Stat-Xplore data (DWP, 2018a) [Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

breaches of conditionality rules, even where it was clear that the claimant had a good reason and was in fact fully co-operating with requirements (Webster, 2016). This issue was then compounded by an increase across the period in the proportion of referrals that resulted in a sanction, which rose from 60 to 80%. As a result of the fluctuations described, during the period there were over 1 million additional JSA sanctions than would have occurred had the rate inherited from the previous government been maintained (Webster, 2016).

## 2.3 | Research questions

Sustained public criticism of sanctions policy following the 2012 reform has motivated several official investigations, which include: the Oakley (2014) review; two inquiries by the House of Commons Work and Pensions Committee (HoC WPC, 2015, 2018); an investigation by the National Audit Office (NAO, 2016); and an inquiry by the House of Commons Public Accounts Committee (HoC PAC, 2017). Each of these investigations have identified various concerns, repeatedly highlighting the paucity of the evidence-base used to inform the increase in sanctions severity in 2012, as well as the lack of a subsequent evaluation into its effects. In this regard, this article focuses specifically on mental health impacts, using available data at the local authority-level to address the following research questions:

Q1. Are JSA sanctions associated with adverse mental health impacts?

Q2. Are the impacts greater following the increase in the severity of sanctions in October 2012?

## 3 | DATA AND METHODS

### 3.1 | Time period

Similar to existing quantitative research into the impacts of benefit sanctions in the UK, the analysis in this article is conducted using quarterly local authority-level data. Specifically, it is restricted to analysing the quarters including and between the third quarter of 2010 and the fourth quarter of 2014 (Q3 2010–Q4 2014). The beginning of the sample captures the rise in sanctioning that occurred at the start of the Coalition government, whilst the pre-2015 restriction minimises the compositional bias introduced by the national rollout of UC. Initially, UC only accepted new claims from single unemployed people deemed to have “straightforward circumstances” (Kennedy & Keen, 2018, p. 13), thereby gradually changing the characteristics of the existing JSA group. In addition, UC sanctions data are only available from August 2015 onwards, creating a gap in sanctions data that is best avoided by the restriction outlined. As previously highlighted, UC began in pilot form in April 2013 in a limited number of local authorities. In order to account for this, the local authority quarters affected are not included in the analysis presented in this article. In a sensitivity check, however, the analysis is re-run using the full sample and the results are substantively similar.

### 3.2 | Outcome variable: Measuring anxiety and depression

Local authority-level rates of anxiety and depression are estimated using the secure access version of the Quarterly Labour Force Survey (QLFS) (ONS, 2018b). The QLFS is a nationally representative, quarterly household survey comprised of approximately 100,000 individuals (ONS, 2016). As Box A1 in online Appendix 2 details, survey respondents are asked if they suffer from any health problems and can select “Depression, bad nerves or anxiety” from a pre-specified list (henceforth, anxiety and/or depression). Bentley, Baker, and Blakely (2016) criticise this self-reported measure due to its subjective nature, which they consider less robust than a diagnostic assessment. The medical diagnosis of anxiety and depression, however, is itself highly contested (Kokanovic, Bendelow, & Philip, 2013). It would therefore be more accurate to recognise that self-reported and diagnostic measures each have

strengths and limitations, without assuming that either benefits from a unique claim to validity. Clearly, the available measure is a crude one in that it ignores the extent to which individuals suffer mental and emotional distress along a spectrum (Pilgrim, 2017). It nevertheless benefits from the fact that it captures self-identified but undiagnosed mental health issues. For the current purposes, it is relatively uncontroversial to assume that increases in rates of self-reported anxiety and/or depression using this measure are indicative of worsening mental health.

Using the secure access QLFS, it is possible to estimate local authority-level rates using the survey weights provided. Area-level estimates using the QLFS are less reliable at lower levels of geography, due to the role of sampling variability from one quarter to the next (ONS, 2016). Following Barr et al. (2016), therefore, quarterly rates of anxiety and/or depression in the working age population are calculated at the county-level of local authority geography, which is more aggregated than the alternative district-level. The single-tiered district system of local governance in the rest of the UK means that the analysis is restricted to the 152 county areas in England. Despite this restriction, the results of the investigation are expected to be similar to what would be found in a Britain-wide analysis. As will be explained, this investigation controls for important differences between local authorities, whilst the Coalition's sanction reforms also applied in Scotland and Wales. The situation in Northern Ireland is distinct, however, and would need to be analysed separately. Sanctions policy is devolved in Northern Ireland, which permits a degree of divergence with UK government reforms. There, rates of sanctions were maintained at a low level throughout the Coalition period, whilst lengthier sanctions were not introduced until April 2016 (Webster, 2018).

With regard to local authority counties in England, the QLFS combines Cheshire East and Cheshire West together, as well as Bedford and Central Bedfordshire, whilst the analysis excludes the City of London and the Isles of Scilly due to their small population size. This means that the results are based on 148 local authority areas through time. Local authority quarters where the QLFS estimates produced zero estimates are removed in the current analysis, though this affected relatively few cases and does not alter the substantive results. Levels of anxiety and/or depression are calculated as rates per 100,000 working age population for each local authority, using mid-year population estimates published through Nomis (ONS, 2018a).

### 3.3 | Explanatory variables: Sanctions and additional factors

JSA sanctions data are sourced from Stat-Xplore (DWP, 2018a), and use is made of the original adverse sanctions measure, as is the norm in the quantitative literature (de Vries et al., 2017; Loopstra, Fledderjohann, Reeves, & Stuckler, 2018). Whilst this measure is the best sanctions rate available for the analysis, it provides an underestimate of the total level of sanctions in any given quarter due to the way that the data are recorded. Despite the fact that JSA is withdrawn whilst a sanction is being appealed, for example, sanctions that are challenged are only recorded at the latest point in time once a decision is made on that case. Rates of challenges are not negligible, at under a fifth of original decisions. As the NAO (2016) highlights, nevertheless, the proportion of imposed sanctions that are challenged remains roughly level during the period of analysis, meaning that the rate used in this investigation provides a consistent measure of sanctions through time. An important implication of this sanctions measure, however, is that the analysis overestimates the relationship between sanctions and anxiety and/or depression. This is formally detailed in online Appendix 3, which indicates that the sanctions coefficients estimated in the results section need to be scaled down by up to a fifth.

Additional local authority-level variables are controlled for, which represent important social determinants of mental health and possible confounding factors (Barr et al., 2016; de Vries et al., 2017; Loopstra et al., 2018; Williams, 2019). Sourced from Nomis, Stat-Xplore, DCLG (2011) and Defra (2014), these include: economic factors (Gross Value Added [GVA], (un)employment and economic inactivity); demographic factors (age, gender, ethnicity); additional social security policies (Work Capability Assessments [WCAs]); and deprivation and rurality. These variables are detailed in Table 1 and are calculated as rates per 100,000 working age population, with the exception of GVA per head, urban-rural classification and Index of Multiple Deprivation quintiles. These latter two variables are

**TABLE 1** Summary statistics for 148 local authority counties (Q3 2010–Q4 2014)

	N	Mean	SD	Min.	Max.	Source
<i>Dependent variable</i>						
Anxiety and/or depression	2,592	5,161	2,255	411	16,959	QLFS
<i>Sanctions variable</i>						
Original adverse sanctions	2,595	434	226	22	1,510	Stat-Xplore
<i>Control variables</i>						
ILO unemployment	2,590	6,141	1,851	1,877	12,694	Nomis
Economic inactivity	2,595	23,428	4,033	13,821	36,524	Nomis
Employment	2,595	69,408	5,168	51,682	81,121	Nomis
Work capability assessments (WCAs)	2,595	439	216	54	1,887	Stat-Xplore
Gross value added (GVA)	2,595	24,903	20,395	12,791	238,714	Nomis
Age						Nomis
16–29 year olds	2,595	29,189	4,493	21,199	43,970	
30–49 year olds	2,595	43,331	3,049	35,066	52,332	
50–64 year olds	2,595	27,480	5,107	12,407	37,484	
Gender (female)	2,595	50,118	827	46,464	52,555	Nomis
Ethnicity (white)	2,595	83,285	15,708	22,145	99,831	Nomis
Index of multiple deprivation						DCLG
Quintile 1	507					
Quintile 2	531					
Quintile 3	516					
Quintile 4	510					
Quintile 5	531					
Urban–rural classification						Defra
Predominantly rural	338					
Urban with significant rural	349					
Predominantly urban	1,908					

Note: Local authority quarters where the QLFS estimates produced zero estimates were removed from the sample.

time-invariant, in that they are measured only once during the period. The results section details how the influence of these two variables is considered within the statistical analysis itself.

Due to data availability, the analysis is not able to include certain relevant control variables. The first is sanctions for Employment and Support Allowance (ESA) claimants, which are the other claimant group subject to sanctions during the period of analysis. ESA is designed to support individuals who are ill or disabled, and those placed in the work-related activity group (WRAG) can be sanctioned for not complying with particular requirements (NAO, 2016). ESA-WRAG sanctions did not occur to the same extent as JSA sanctions throughout the period, however, and did not correlate with JSA sanctions in a manner such that their exclusion is likely to bias the results obtained here (Webster, 2016). The second is hardship payments, which provide an institutional route for receiving financial support for sanctioned claimants. For those who can demonstrate that they are at risk of financial hardship, payments are available at a rate of 60% of JSA for the majority of claimants who are not deemed “vulnerable,” which begin in the third week of the sanction period. Under half of sanctioned claimants typically receive such support, with rates generally below 10% prior to 2013, a figure which rose to over 40% by the end of 2015 (Webster, 2015).

### 3.4 | Statistical approach

The current analysis is carried out at the local authority-level, and it is therefore necessary to avoid committing an “ecological fallacy” when interpreting the results. As Pearce (2000) argues, nevertheless, aggregate-level studies have an important role to play where individual-level data are unavailable, given that they can provide initial tests of developing hypotheses and can identify important policy issues that warrant further investigation. Exploiting local authority-level variation in quarterly rates of JSA sanctions and anxiety and/or depression through time, the investigation estimates the relationships of interest using the following fixed effects regression model:

$$\text{Anxiety}_{i,t} = \beta_0 + \beta_1 \text{Sanctions}_{i,t} + \beta' X_{i,t} + Q_t + \mu_i + \lambda_t + \varepsilon_{i,t} \quad (1)$$

In the above equation,  $i$  represents the local authority,  $t$  represents the quarter and the dependent variable Anxiety is the rate of anxiety and/or depression per 100,000 working age population. The key independent variable of interest is Sanctions, the rate of JSA sanctions per 100,000 working age population, whilst  $X$  indicates the additional explanatory variables.  $Q$  is a dummy variable coded 0 for quarters prior to Q2 2013 and 1 thereafter, which takes into account the slight change in the QLFS survey question at that point in time (see online Appendix 2, Box A1). Both local authority fixed effects ( $\mu$ ) and time fixed effects ( $\lambda$ ) are included; the former account for initial differences between local authorities whilst the latter account for factors that affect all local authorities through time.  $\varepsilon$  represents the error term. The key coefficient of interest is  $\beta_1$ , which estimates the relationship between increases in the JSA sanctions rate and the rate of anxiety and/or depression within local authorities. All models are estimated using Stata v15 and use standard errors that are robust to heteroscedasticity, cross-sectional and serial correlation (Hoechle, 2007).

The analysis proceeds in two stages. The first stage addresses the first research question by considering the relationship between sanctions and anxiety and/or depression within local authorities across the full time period. The second stage addresses the second research question by considering the influence of the JSA sanctions reform in October 2012. As previously highlighted, this reform increased both the minimum and maximum length of JSA sanctions, meaning that the sanctions data used in this analysis are qualitatively different in the latter period. To take this into account, the initial fixed effects model is augmented in the second stage, through the inclusion of an interaction between Sanctions and a dummy variable Reform that is zero in the quarters prior to the reform and one from Q4 2012 onwards:

$$\text{Anxiety}_{i,t} = \beta_0 + \beta_1 \text{Sanctions}_{i,t} + \beta_2 (\text{Sanctions}_{i,t} * \text{Reform}_t) + \beta' X_{i,t} + Q_t + \mu_i + \lambda_t + \varepsilon_{i,t} \quad (2)$$

In the above equation, the post-reform relationship between sanctions and antidepressant prescribing is then arrived at through calculating a linear combination of  $\beta_1$  and  $\beta_2$ . This is done using the Stata command “lincomest” created by Newson (2002). The remainder of this chapter details the results of these fixed effects regressions and then goes on to discuss the implications of the findings obtained.

## 4 | RESULTS

### 4.1 | Stage one results

As Table 1 details, within the local authority sample there are an average of 434 JSA sanctions and 5,161 people experiencing anxiety and/or depression per 100,000 working age population per quarter. Some local authorities have high rates for both factors. Hartlepool, for example, has an average of 735 sanctions and 8,433 people experiencing anxiety and/or depression per 100,000 working age population per quarter. Elsewhere, the relationship is different. East Sussex, for example, has a low rate of sanctions but a relatively high rate of people experiencing anxiety and/or depression, which indicates that factors other than sanctions are important. This is confirmed in the scatterplot in

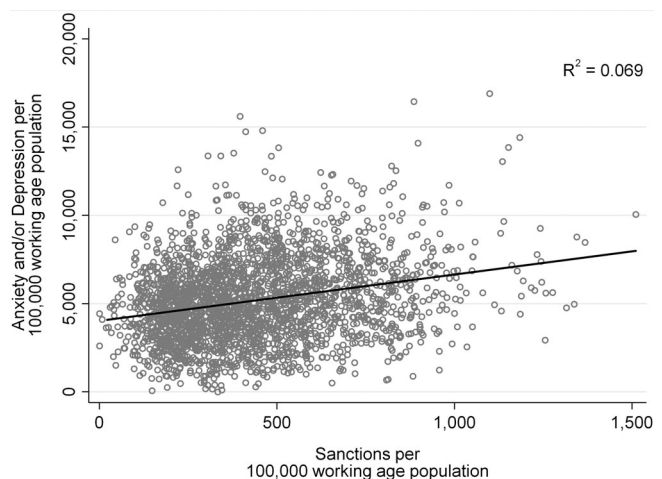


Figure 2, which depicts the correlation between sanctions and anxiety and/or depression in each local authority quarter across the period. It indicates that sanctions are positively associated with anxiety and/or depression at the local authority-level, though this correlation is a relatively weak one ( $r = .263$ ;  $p < .001$ ). This motivates the estimation of fixed effects models to consider the influence of additional factors.

In the initial stage of the modelling process, two fixed effects models were estimated that are detailed in online Appendix 4 (Table A2). The process of model selection will be briefly described here, before the results are discussed in more depth. In Table A2, in addition to sanctions, Model A1 includes various time-varying local authority factors. Model A2 repeats this model but also includes Index of Multiple Deprivation quintiles and urban–rural classification, each interacted with a linear time term. Time-invariant factors such as deprivation and rurality are perfectly collinear with the local authority fixed effects, though their inclusion using separate time interactions captures how their influence on anxiety and/or depression changes over the period (Barr et al., 2016). This is a potentially important dimension to capture due to, for example, the uneven impact of austerity policies at a local level during the period of analysis (Gray & Barford, 2018; Thomson, Niedzwiedz, & Katikireddi, 2018). The key coefficient of interest for sanctions is similar across the two models, though Model A2 is ultimately preferred due to the slight increase in within- $R^2$  and the significance of the additional variables. Here, Model A2 is repeated as Model 1 in Table 2. A full set of regression diagnostic checks for this model are detailed in online Appendix 5.

The results estimated in Model 1 support the view that sanctions are associated with adverse mental health impacts. They indicate that, for every 10 additional sanctions applied per 100,000 working age population per quarter, the rate of anxiety and/or depression increases by 7.77 per 100,000 working age population. This result is statistically significant at the 0.1% level ( $p < .001$ ) (95% CI: 3.57–11.97). With regard to the additional explanatory variables, several of these conform to the expected relationship with rates of anxiety and/or depression. In terms of the statistically significant results, increases within local authorities in rates of economic inactivity are associated with increases in anxiety and/or depression, whilst increases in GVA are associated with decreases. A positive relationship exists, furthermore, in terms of rates of WCAs and females, though these results are non-significant at the 5% level.

The negative and close to zero coefficient for unemployment is, however, surprising. The correlation between unemployment and sanctions is relatively high ( $r = .653$ ), though separate regressions with and without each variable included indicate that multicollinearity is not affecting the Model 1 estimates. The analysis is also re-run using the JSA claimant count, as opposed to ILO unemployment, though similar results are again observed. One possible explanation of this outcome derives from the fact that unemployment was relatively stable throughout the period, given that fixed effects models can struggle to accurately estimate the influence of variables with low within-unit variation



**FIGURE 2** Relationship between JSA sanctions and anxiety and/or depression, scatterplot

*Note:* Quarterly rates for 148 local authority counties Department for Communities and Local Government (DCLG), 2011 Department for Environment Food and Rural Affairs (Defra), 2014

**TABLE 2** Relationship between sanctions and anxiety and/or depression, fixed effects regressions

	Model 1	Model 2
Sanctions	0.777*** (0.199)	0.690 (0.459)
Sanctions × reform		0.119 (0.515)
Unemployment	−0.009 (0.041)	−0.009 (0.041)
Economic inactivity	0.074** (0.020)	0.074** (0.020)
WCAs	0.216 (0.536)	0.212 (0.549)
GVA	−0.059*** (0.014)	−0.060*** (0.013)
Age		
30–49	−0.407*** (0.104)	−0.404** (0.109)
50–64	0.105 (0.108)	0.104 (0.109)
Female	0.504 (0.473)	0.498 (0.467)
White	0.025 (0.019)	0.025 (0.019)
Index of multiple deprivation		
Quintile 2 × quarter	51.541** (14.751)	50.416** (16.222)
Quintile 3 × quarter	56.931*** (6.926)	54.829*** (12.786)
Quintile 4 × quarter	66.666*** (16.652)	63.880** (18.871)
Quintile 5 × quarter	80.834** (21.423)	76.581* (29.328)
Urban–rural classification		
Urban with significant rural × quarter	10.867 (12.300)	10.519 (13.054)
Predominantly urban × quarter	−50.008** (14.758)	−50.531** (15.344)
R <sup>2</sup> (within)	0.847	0.847
LA quarters	2,587	2,587

Note: Robust standard errors in brackets. Models include local authority and time fixed effects. Constant and LFS question change dummy not shown.

\* $p < .05$ , \*\* $p < .01$ , \*\*\* $p < .001$ .

(Mummolo & Peterson, 2018). Random effects models provide an alternative means of capturing the effects of such slowly changing variables, and so the analysis is re-run using random effects estimation (see online Appendix 6, Table A4). In Table A4, the coefficient for unemployment is now positive as expected, though it remains non-significant at the 5% level. This exercise nevertheless provides a useful robustness test on the main Model 1 sanctions result, given that the sanctions coefficient is similar across the fixed and random effects specifications. This indicates that the substantive findings are not driven by the primary use of fixed effects.

## 4.2 | Stage two results

The next stage of the analysis takes into account the increased severity of sanctions from October 2012 onwards, as detailed in Model 2 (Table 2). Importantly, the results indicate that the mental health impacts of sanctions are stronger following the reform. In the pre-reform period, for every 10 additional sanctions applied per 100,000 working age population per quarter the rate of anxiety and/or depression is 6.90 per 100,000 working age population higher. This result, however, is not significant at the 5% level ( $p = .151$ ). Following the reform, the sanctions coefficient increases by 1.19 per 100,000 working age population, such that for every 10 additional sanctions applied per 100,000 working age population per quarter the rate of anxiety and/or depression is 8.09 per 100,000 working age

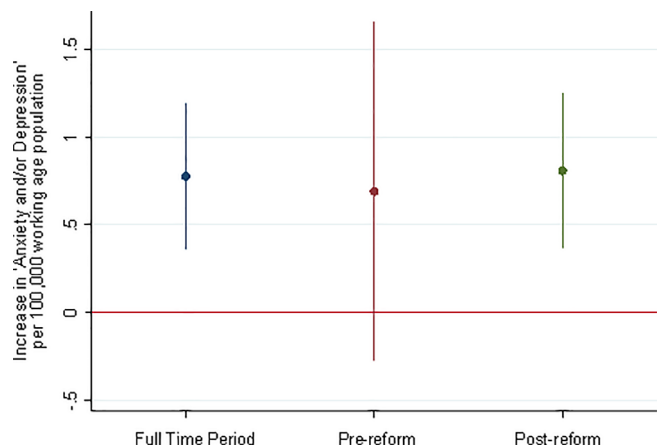
population higher. This result is significant at the 0.1% level ( $p < .001$ ) (95% CI: 3.67–12.50). The results of both stages of the analysis are depicted in Figure 3.

At this juncture, it is useful to briefly consider the plausibility of the results obtained, particularly in terms of the size of the estimated sanctions impact in the post-reform period. The possible concern is that the effect size is implausibly high, which might be indicative of some form of bias. As highlighted in Section 3, however, this coefficient needs to be scaled down by up to a fifth due to the available sanctions measure that is used in the investigation. In addition to this technical adjustment, a relatively high sanctions coefficient is arguably expected given that the analysis is carried out at the local authority-level. Arguably, the use of aggregated data means that the results do not solely capture imposition effects on JSA claimants. Previous qualitative research indicates that the emotional and psychological impacts of sanctions can extend beyond claimants to family and friends (Dwyer, 2018), which will be captured in the current area-level analysis. The mental health of claimants who receive a sanction, furthermore, is also likely to have been impacted by the preceding threat of having such a financial penalty imposed (Dwyer et al., 2020).

### 4.3 | Sensitivity and robustness checks

Additional sensitivity and robustness checks are carried out. First, a check for omitted variables bias is conducted using a non-equivalent dependent variable (Coryn & Hobson, 2011). The fixed effects models used in the main analysis control for time-invariant omitted factors and include several additional explanatory variables, though the influence of unobserved confounders cannot be completely discounted. The non-equivalent dependent variable approach investigates an alternative outcome that is not expected to be impacted by sanctions but that might be impacted by similar unobserved factors affecting the main analysis. Here, the rate of anxiety and/or depression per 100,000 population aged 65+ is used as the alternative outcome, as this population is not at risk of being sanctioned. It may, however, be influenced by similar omitted factors such as changes in willingness to report health problems or changes in access to healthcare. Importantly, the main results are supported by the fact that no statistically significant relationship with sanctions is found when using the alternative outcome (see online Appendix 7, Table A5). Whilst a positive relationship is observed, the estimated sanctions coefficient is in fact lower in the post-reform period.

Second, the potential issue of reverse causality is investigated in a Granger test, using the Stata command “xtgcause” created by Lopez and Weber (2017). Using a lag structure of four quarters, sanctions are found to Granger-cause anxiety and/or depression ( $p = .013$ ), whilst rates of anxiety and/or depression are not found to Granger-cause sanctions ( $p = .608$ ). These results support the causal inferences made throughout the investigation,



**FIGURE 3** Relationship between sanctions and anxiety and/or depression, sanctions coefficients

Note: Sanctions coefficients are derived from Table 2. Vertical bars represent 95% confidence intervals [Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

which are based on the independent effect of sanctions on mental health. Indeed, this interpretation is arguably strengthened by the fact that the impact of sanctions is observed to increase following the October 2012 sanction reform, which is a result that is harder to explain with an alternative causal explanation. At the individual-level, of course, it could be the case that the direction of causality runs both ways. That is, whilst sanctions are expected to have an independent effect on mental health, claimants already suffering from mental health problems might also be more likely to receive sanctions in the first place.

## 5 | DISCUSSION AND CONCLUSION

This investigation provides robust evidence that sanctions are associated with adverse mental health impacts, measured in terms of self-reported anxiety and/or depression at the local authority-level. The central finding is that, following the increase in the severity of JSA sanctions in October 2012, every 10 additional sanctions applied per 100,000 working age population per quarter are associated with approximately eight additional people reporting that they suffer from anxiety and/or depression. As previously explained, this headline result overestimates the relationship between sanctions and anxiety/depression, which may need to be scaled down by up to a fifth. The finding is nevertheless considered plausible, given that the lengthier sanctions introduced in October 2012 appear to have had no impact on flows from JSA into employment (Taulbut et al., 2018), though did have adverse effects in terms of increased rates of food bank usage and antidepressant prescribing (Loopstra et al., 2018; Williams, 2019). The study therefore provides valuable quantitative evidence that supports the limited existing literature, both in terms of the wider impacts of sanctions more generally but also in specific relation to the 2012 sanctions reform in the UK.

Importantly, various steps have been undertaken to minimise the influence of bias on the results obtained. This began with the time period selected for the analysis, which is restricted to before the national rollout of UC in order to avoid the compositional change that this reform introduced for the JSA group. The investigation itself makes use of fixed effects models that control for differences between local authorities, in terms of both time-invariant omitted factors and time-varying included factors. The analysis is also supported by various sensitivity checks in terms of model selection, as well as robustness checks for omitted variables bias and reverse causality.

Despite these efforts, it is necessary to highlight that important limitations remain. In addition to the use of observational data, the central restriction is that the investigation is carried out at the local authority-level and therefore cannot avoid the risk of ecological bias. Associations that hold at the area-level do not necessarily apply at the individual-level, and so a key implication of this research is to motivate additional individual-level research into the mental health impacts of benefit sanctions. With this caveat recognised, it is nevertheless important to highlight that the area-level findings from this study are consistent with existing qualitative research undertaken with claimants (Dwyer, 2018; Dwyer et al., 2020; Jamieson, 2020; Wright et al., 2020). Indeed, the findings are at least indicative of individual-level impacts, since it is not immediately clear why such relationships would hold at the area- but not at the individual-level.

The DWP has recently committed to sharing administrative data on sanctions with researchers for the purposes of investigating health impacts (HoC WPC, 2019). Future individual-level research should utilise appropriate research designs to consider the complex causal relationships involved in this area, as well as investigate scalar measures of mental distress such as the GHQ-12 that were not available here (Brown, Harris, Srivastava, & Taylor, 2018). As previously highlighted, it is necessary to distinguish the independent impact of sanctions on mental health outcomes from the impact of prior mental health status on the likelihood of being sanctioned. Each effect has negative implications in terms of the functioning of sanctions policy, though it is useful to distinguish between the two in order to develop effective policy responses. An important additional topic is the interplay between mental health impacts and labour market outcomes. The initial mental health impacts of sanctions may affect claimants' attempts to search for and secure employment (Dwyer et al., 2020), whilst longer-term impacts may depend on the quality of jobs for those that find work (Chandola & Zhang, 2018).

The results of this study provide useful evidence for policymakers internationally, where a general trend has been observed towards the implementation of increasingly stricter sanctioning policies (Knotz, 2018). The increases in anxiety and depression observed in this research indicate that punitive responses to benefit claimants can serve to worsen the impact of unemployment on mental health. This indicates that there is a need to re-consider the overall efficacy of conditionality requirements and sanctions against alternative measures designed to support claimants into work (Dwyer, 2018). A potential option would be to expand the use of interventions collectively termed active labour market policies (ALMPs), such as intensive skills and training schemes. Research indicates that well-designed ALMPs can, in addition to improving employment prospects, partially protect claimants against the mental health and well-being impacts of unemployment (Crost, 2016; Puig-Barrachina et al., 2020; Sage, 2015; Wulfgramm, 2011).

With regard to sanctions policy in the UK, the findings hold important implications for the functioning of UC, given that the new benefit is based on the escalating sanctions regime that pertained under JSA (Millar & Bennett, 2017). Indeed, recent evidence links the introduction of UC to an increase in psychological distress and depression amongst claimants, with UC sanctions policy identified as a potential driver (Wickham et al., 2020). Two broad areas of concern are implicated given that the results of this study are driven by both the severity and scale of sanctions in the post-reform period.

First, there is an evident need to reduce the severity of sanctions, in terms of both their length and the proportion of benefit that is removed. In a recent positive development, the DWP has ended the use of sanctions lasting 156 weeks (HM Government, 2019). Sanctions are consecutive under UC, however, which implies that some claimants will be affected by benefit withdrawals that exceed the new 26 weeks maximum. A related issue is the functioning of the hardship payments system, previously outlined in Section 3. Webster (2014) highlights that the DWP Decision Makers' Guide (2009, para. 35099) already anticipates that the 2-week waiting period for a hardship payment following a sanction can be expected to cause a "healthy adult to suffer some deterioration in their health". Indeed, the findings from this investigation are observed even though the proportion of sanctioned claimants receiving hardship payments increased in the post-reform period. Under UC, hardship payments are awarded for fewer reasons and are repayable. These changes are likely to explain the fact that less than 20% of sanctioned claimants receive such payments within UC, which is under half the proportion that had pertained under JSA (Webster, 2019).

Last, is the issue of the overall application of sanctions. Monthly UC sanctions have varied in the course of the rollout, falling from a high of approximately 9% of claimants in 2015 to 2% by 2019 (Webster, 2019). The present research suggests that the DWP should consider options to maintain this low rate, such as implementing a sanctions warning system, limiting the number of reasons for which sanctions apply and/or establishing a more lenient conception of "good reason" for which benefit rules might be contravened (Adler, 2018). Considering the growing evidence into mental health impacts, to which this study contributes, the need to implement straightforward measures to limit the severity and scale of sanctions is evident. Indeed, this need is reinforced by the deficiencies in the positive case for sanctions in terms of labour market effects, which implies that UK sanctions policy should take a precautionary and harm reduction approach in future.

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## CONFLICT OF INTEREST

The author declares no potential conflict of interest.

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## SUPPORTING INFORMATION

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